

VOLUME V. }
NUMBER 2. }

NOVEMBER, 1887.

Annual Subscription
to Institution only \$5.00
Single Copies . . 50 cts.

TRANSACTIONS
OF THE
AMERICAN INSTITUTE
OF
ELECTRICAL ENGINEERS.

SPECIAL MEETING, OCTOBER 11, 1887.

SUBJECT:

A Coulomb-meter, or Instrument for measuring the Consumption of
Electricity.

BY PROF. GEORGE FORBES, F. R. S.

NEW YORK CITY:
PUBLISHED BY THE INSTITUTE.
R. W. FARR, Secretary.
111 DAY STREET.

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AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

VOL. V. November, 1887. No. 2

SECRETARY'S BULLETIN.

Volume 4 of the Transactions for the years 1886-7 has been issued and mailed to all members and associates in good standing. It contains 232 pages.

The proceedings of the annual business meeting will be printed in a separate volume, which is now in press.

The next meeting of the Institute will be held on November 9th, in the Managers' room, at the American Institute Exhibition building, Third avenue, between 63d and 64th streets. A paper will be read by Mr. Charles Cuttriss, Electrician of the Commercial Cable Company, on "Recent Improvements in Apparatus for Ocean Telegraphy." This meeting will be held in conjunction with the Electrical Section of the American Institute, formerly the New York Electrical Society, and tickets of admission to the general exhibition will be issued.

Further details of the arrangements will be furnished by a circular mailed before the meeting takes place.

At the meeting of Council, held September 6th, the following gentlemen were elected to associate membership of the American Institute of Electrical Engineers:

F. G. Waterhouse, Hartford, Ct.	F. R. Welles, Antwerp, Belgium.
S. C. Jones, Aurora, N. Y.	G. G. Wacker, New York.
Carlos M. e Souza, Para, Brazil.	A. M. Young, Waterbury, Ct.
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F. E. Fielding, Virginia, Nev.	E. W. Garrette, Woodland, Cal.
(Life Associate Member.)	Alex. Anderson, Charlottetown,
F. L. Elliott, Oakland, Cal.	P. E. I.
H. A. Ockershausen, Brooklyn, N. Y.	Theodore Mace, New York.
W. H. Childs, Brattleboro, Vt.	E. C. Meilloret, New York.
J. F. Porter, St. Louis, Mo.	J. W. Lieb, Jr., Milan, Italy.
Allen C. Knapp, Chicago, Ill.	W. Van Brunt, Duluth, Minn.
B. C. Batchellor, Fort Hamilton, N. Y.	Almon Robinson, Lewiston, Me.
J. Stanford Brown, New York.	J. H. Bates, Hoboken, N. J.
Lieut. F. J. Patten, Fort Sidney, Neb.	S. C. Peck, Newtonville, Mass.
E. R. Weeks, Kansas City, Mo.	C. J. R. Humphreys, Lawrence, Mass.
	J. Beattie, Jr., Fall River, Mass.

The following associate members were, upon recommendation of the Board of Examiners, transferred to full membership:

Dr. Louis Duncan, Baltimore, Md.	G. H. Condict, San Francisco.
R. N. Dyer, New York.	Elias E. Ries, Baltimore, Md.
H. Ward Leonard, Chicago.	T. Ahearn, Ottawa, Ont.
George W. Blodgett, Boston.	R. J. Pratt, Greenbush, N. Y.
	H. A. Foster, Lynn, Mass.

At the Council meeting of October 4th the following gentlemen were elected associate members:

Wm. Standford, Cape Town, Africa.	Herbert G. Pratt, West Newton, Mass.
B. Bayly, Cape Town, Africa.	H. J. Ryan, Lincoln, Neb.
R. G. Brown, New York.	Dr. H. F. R. Hubrecht, Amsterdam, Holland.
William P. Maycock, London, Eng.	D. E. Goe, Memphis, Tenn.
Prof. Edward L. Nichols, Cornell University, Ithaca, N. Y.	W. A. Kreidler, Chicago, Ill.
Ernst Thurnauer, Paris, France.	Henry J. Mulholland, Jr., Probolingo, Java.

The Council extends an invitation to all members and associates to contribute papers for discussion at the meetings of the Institute. As arrangements are now made for two months in advance, ample time may be given to their preparation.

Readers detecting errors of any character in this pamphlet will confer a favor by notifying the Secretary at once, so that corrections can be made before printing the sheets for the annual volume.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

SPECIAL MEETING.

Held at the House of the American Society of Civil Engineers,
127 East 23d street, New York, October 11th, 1887.

The meeting was called to order at 8 P. M. by the President of
the Institute, Mr. T. C. MARTIN.

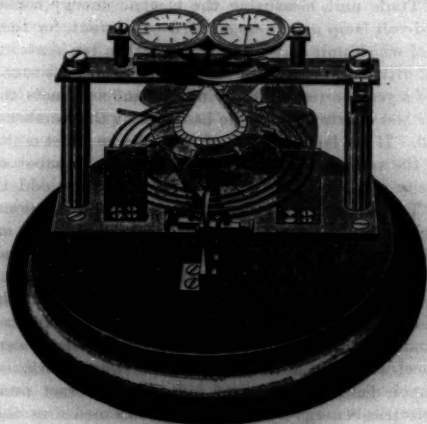
THE PRESIDENT: I need hardly say, gentlemen, that all of us have great pleasure in welcoming here this evening so distinguished a scientist and so well-known an electrician as Professor George Forbes, of England. Some of us have already had the pleasure of listening to Professor Forbes in this country, and remember the occasion of his visit here in 1884, at the time of the National Conference of Electricians and the International Electrical Exhibition in Philadelphia. Professor Forbes is also known to us through his work in England and as the inventor of a number of highly ingenious devices, and I think by the close of the evening, you will all agree with me that in none of his inventions is greater ingenuity and grasp of electrical principles displayed than in the apparatus which he brings before us this evening. This instrument is one for which there has been a decided want, and it brings before us questions of the utmost importance and pre-eminence at the present time. I have no doubt that both the paper and the discussion upon which we shall enter this evening will do much to start us far along a new road of original investigation. Nothing for me remains, therefore, but to introduce to you Professor Forbes, who will now read to you his paper on a Coulomb-Meter.

A COULOMB-METER, OR INSTRUMENT FOR MEASURING THE CONSUMPTION OF ELECTRICITY.

BY PROF. GEORGE FORBES, F. R. S.

You all remember that in England, at the time when electric lighting took a decided start, there was a regular rush for illuminating towns by central station lighting. I looked into the matter at that time, and could not see that a single scheme that was then proposed was capable of being worked. We were at that time absolutely ignorant of the methods of distribution of electricity. In the first place, we were absolutely ignorant of what amount of current we might pass through wires of different sizes. One had to go all through that, and work out the problem before starting upon anything else. Then the different systems of distribution proposed had to be discussed in each particular case, to see which was the most useful, and perhaps you will remember that I myself came to the conclusion at that time that a direct supply on the multiple arc system, which was then most in vogue, could not be carried out on economical lines to a greater distance than 220 yards, and when the three wire system, the next decided advance, was introduced, it extended the distance of economical working to about a quarter of a mile. The greatest step, it seems to me, which has been taken in the distribution of electricity since the period when electric lighting took such a start in 1878, was the use of alternating currents with induction apparatus, which was simply achieved by the most indefatigable industry and plucky perseverance against the opinions of everybody who seemed to be capable of giving an opinion. I have never omitted, on any occasion when I had to speak about the distribution of electricity, to express the profound admiration I have had for the energy and pluck which was shown by Messrs. Gaulard and Gibbs in working out that system upon which so much ridicule was cast during the whole time they were making their experiments. At the present moment the only difficulty which has seemed to exist in the way of a general system of distribution lies in the want of a meter for the purpose. It is all very well to do as we are doing just now, to charge people for lighting their houses the same price that they used to pay for gas, or to charge them at so much a lamp and let them burn all night long or just as much as they please; that is

all very well as a makeshift, but every one of us knows perfectly well that it cannot go on forever; and therefore we must have a meter for measuring the consumption of electricity in houses. With a direct or continuous current, such as has been generally used on the ordinary multiple arc or the three-wire system, the problem is not a very difficult one, and has been solved with greater or less simplicity, with greater or less accuracy, in a variety of ways. But even in that system it has always been felt that there was great room for improvement over anything which has been done. In using alternating currents, however, there has been, until lately at any rate, absolutely no meter in existence which was capable of giving, with even a fair degree of accuracy, the consumption of electricity in a house.



THE FORBES COULOMB METER.

Nearly a year ago an American in England spoke of the thing as impossible, and defied me to produce one. That same evening I went home and tried a simple experiment which convinced me I had found the solution of the problem. Of all the effects of the electric current I was sure that the only one which would suit alternate currents was the creation of heat in a conductor. The experiment I tried was to cut a circular disk of paper into a spiral and to support this spiral at its center on a needle point. Holding this in an upward draught of air, the spiral rotates. I tested

this and found that a very feeble amount of heat sufficed to produce this effect. I then put some wire under the spiral and passed an electric current through it, and found that the spiral moved with an extremely small consumption of electric energy. From this beginning, after nine months' incessant work, I evolved the coulomb-meter which I now exhibit. From this work has resulted the simplest and most accurate meter conceivable, which is equally suitable for continuous or alternate currents, which is read off by indexes and dials like a common gas-meter, and which, when set in place, never needs to be touched, the readings being taken simply from the dials.

There are four dials in the latest instruments, reading units, tens, hundreds and thousands. The unit I adopt is the English Board of Trade unit, measuring the electric energy consumed in a hundred-volt lamp using one ampere of current for ten hours, the cost of which unit may be a little less than ten cents in a good system of distribution. When the inspector goes his rounds each quarter of a year, he reads off the indexes and subtracts the reading of the last quarter, and so he has directly the number of units consumed. If the lamps be not of 100 volts he must multiply his result by the volts and divide by 100 to get the number of units of electricity used up in the quarter year. I might add that the Board of Trade unit is equivalent to a thousand revolutions of the mill-wheel. This instrument is made to work up to 20 amperes, and it might work 20 hours a day for four or five months before the readings of the dials would be exhausted.

I believe that the meter which I present before you to-day is as simple in construction as any meter can be which requires wheel-work to indicate the amount of electricity which has been used. It has wheel-work just as a gas-meter has wheel-work, so as to indicate upon dials, by means of indexes, the exact number of units of electrical energy; just as a gas-meter measures the exact number of feet of gas which has been consumed. But in making this comparison between the gas-meter and the electric-meter you must not imagine that they are to be put upon a par with each other. The gas-meter has become a proverbial instrument, not for the great accuracy with which it performs its work, but for totally different qualities from which I trust this meter before you is wholly free.

The instrument is ridiculously simple, and consists of four parts: (1) A mill-wheel (or smoke jack, as Sir Frederick Bramwell has called it); (2) a wire under the mill-wheel, which creates an upward current of air when the electric current goes through it

and beats it; (3) wheel-work for counting the number of revolutions of the mill-wheel; and (4) dials and indexes for reading off the number of revolutions.

The mill-wheel consists of eight mica vanes fixed by pieces of pith to the circumference of a circular disk of mica, at an angle of forty five degrees to the horizon. This disk of mica has a circular hole in the middle, where it is supported by a thin paper cone, apex upward. At the top of the cone there is a jeweled cup fixed inside a pinion. The jeweled cup rests on a point fixed to the base of the instrument.

The wire which is to be heated by the electric current is of iron and forms a flat spiral of four turns just underneath the vanes of the mill-wheel. The ends of this wire are attached to one of the wires carrying electric currents to the lamps.

The pinion on the cone of the mill-wheel works in a train of wheels, so as to reduce the speed. One thousand revolutions of the mill-wheel indicate one unit on the dials.

Each of the dials has ten divisions. The entire instrument is placed under a cover, to keep off dust and irregular currents of air.

The instrument as now made starts with half an ampere and is fairly accurate with three-quarters of an ampere (the amount of current generally used in an incandescent lamp). From two lamps up to thirty lamps, the speed of revolution is exactly proportional to the current passing.

The same identical instrument will indicate from 20 to 600 lamps with the same accuracy. This is done by diverting part of the electric current from the meter by a shunt wire, so that only one twentieth part of the whole current is measured.

If I want to measure from one lamp to six hundred lamps, I use two meters, one with and the other without the shunt. With small currents only, the meter for thirty lamps is connected. When the current for twenty lamps is passing, it is strong enough to act on an auxiliary apparatus which allows the greater part of the current to pass through the second meter. [The two meters are then in parallel.] This auxiliary apparatus is an electro-magnet with a coil of twenty turns of wire going to the first meter and one turn going to the second. When the current is strong enough it pulls down a lever, which makes connection with the second meter and holds it there as long as it is wanted.

The want for such a meter has long been felt. Neither consumers nor suppliers of electric light and motive power can be satisfied with a price determined solely by the number of lamps in

a house, and not by the hours of consumption. The meter must be simple and cheap, and must be readable by the consumer as well as by the supplier of electricity. These conditions are all fulfilled by the meter I am exhibiting to you.

In isolated plants, where storage batteries are used, this instrument enables us to see how much electricity is put into the batteries and how much they give up.

In distributing motive power by electricity it is quite impossible to charge the consumer by the number of motors, because the time he is actually running the motors may be very variable. A meter is indispensable.

I may now refer you to the table on the blackboard, showing the indications of the instrument, from which you will see that the ratio of speed to current is practically constant:

Current in amperes	.25	.35	.45	.6	.75	1	2	3	6	12
Ratio $\left\{ \frac{\text{current}}{\text{speed}} \right\}$	76	61.2	50.4	51	50.7	51	51	50.7	51	51.6

I cannot conclude this description without acknowledging the generous enthusiasm, free from all jealousy, with which this meter was received by English electricians when I showed it for the first time—namely, last month—at the meeting of the British Association at Manchester. I must say that the kind sympathy of these eminent men went to my heart. May I be allowed to quote the remarks of one or two from the *London Electrical Review*, September 23?

Sir William Thomson said, "I have very great pleasure indeed in congratulating Professor Forbes. Every one who knows anything about electric lighting understands perfectly that the want of a meter is one of the greatest practical wants in connection with the introduction of central station lighting. I speak feelingly on the subject, for I have tried year after year to produce a meter. There is nothing to which I have given up so much time in endeavoring to make an instrument work, as in endeavoring to produce an electric meter, and the Patent Office contains records of some of my attempts. Each one of them has been to some degree capable of giving a result, and the last of them, which I worked at a very great deal till four months ago, promised considerable accuracy, and was perfectly available for either direct or alternate currents. But the difficulty of seeing a way to bring this instrument out and to make it a practical

thing, at a moderate cost, for general use, was so great that I do not feel that I intend to go on with it at all. It is just possible that I might make it succeed as a meter for a large station. But I am exceedingly glad to see the great success which Professor Forbes has obtained. I think I may say I am untinged with any feeling of regret at its having been done by another. On the contrary, it is with the most unmixed feelings that I say I am exceedingly glad, and that Professor Forbes is to be congratulated."

Professor Perry said, "One has no right to ask an inventor of such a beautiful instrument as this by what means he arrived at the idea embodied in it. I could not have imagined that he could have got this result. The figures certainly show that it is a beautifully simple instrument, and effects its purpose well. But how any one could arrive at such a method from any theoretical information passes my understanding. The whole action seems to be so complicated. I do not think I should have tried it if the idea had come into my head. I suppose one must miss a tremendous lot of valuable things in this way, because one does not see the theory. It is a marvelous thing. I certainly think it is one of the most beautiful things we have had before us for a long time. I speak now as an inventor of a meter which is in considerable practical use, not in this country, unfortunately, where it is patented, but in Germany. That is also a very simple one, but I think that this beats it."

I will not trouble you with further quotations. I can only say that I was deeply touched by the sympathy of other inventors, and that the reception I met with in England led me to take this opportunity of showing the instrument to you on this side of the Atlantic.

DISCUSSION.

PROFESSOR FORBES: I would like to show you one or two points about the meter. In this instrument there is one part which I have not described to you. Perhaps before doing so I ought to let you see it working. I will put on a battery current and we will make it go round and see what happens. I am now starting it with a current of eleven amperes.

A MEMBER: When you break the current how soon does it stop?

PROFESSOR FORBES: It takes a little time to get up its full speed, and a little time to lose its full speed. I have shown this lighter form of wheel-work, and I am making it even lighter than this is in the instrument which is now being manufactured. This instru-

ment has a good deal of metal work—much more than in any of the instruments I have previously used, and the mass of metal absorbs so much heat that it goes on for a considerable time after the current is broken. Now this might suit the suppliers of electricity very well, but it would not, of course, suit the consumers, and one must defer to both sides. I do not want the meter to act in that way, and I find that by reducing the metal work as much as possible that difficulty is removed. There is another point about this instrument which I have not shown you or spoken about. It is an arrangement which is attached to it for lifting up the mica vanes against a fixed stop above. I have moved the stop away, you see, at present. That attachment lifts the mill-wheel up against the stop and enables us to carry the instrument about with safety, under ordinary circumstances. I must make one reservation—there is fear of danger if it passes through the New York custom-house. (Laughter.) I brought in a lot of instruments, and after I saw a little of what the custom-house people were doing, it sickened my heart so much that I let them lock the instruments up and put them in the public stores. I had worked six hours that morning like a slave doing their bidding, and by the time they came to me I am sorry to say that many of the things were in a sad state. You will notice something hanging to that instrument. That was an extremely delicate spiral spring, which was attached to one of these mill-wheels, acting simply as an ampere meter, not acting for registering the consumption, but measuring the rate at which the current is passing through the conductor. That instrument is quite disabled, and others, too. I have also some diagrams here. Here are actual examples of some of the readings of these ampere-meters, and I have sometimes taken them under different conditions. I have, for example, the instrument with the clock-work in, and without the clock-work, showing the amount of friction; and I may tell you this, that it looks as if the wheel-work there would cause a considerable amount of friction, which has to be overcome. That friction is a mere nothing compared with the friction of the air which is circulating in these convection currents which are created. There are three frictions coming in: First, there is the air friction in the currents of air, which is the chief source of friction; secondly, there is the friction of the jeweled cup on the point; and, lastly, there is the friction of the wheel-work, which is far less than the friction of the jeweled cup upon the point. That is what one of these diagrams shows. Another one happens to show the effect when the apparatus for fixing the mill-wheel is taken

out. Another one may be of some interest to you. It was this instrument when it was in the experimental stage. I varied the distance of the vanes from the conductors and I used it at $\frac{1}{16}$, $\frac{1}{8}$, $\frac{1}{4}$, and $\frac{1}{2}$ of an inch. You might expect that the nearer it was to the conductor the greater the velocity would be of the given current. That is not so. It then creates friction by pulling the air from the surface of the wire when it is too near. Three-sixteenths of an inch was found to be the most suitable distance. There is one peculiarity of this instrument which I believe no other meter has. I have not only my own tests of it, but I have also some independent tests. I have never been able to get any independent tests of any meter in the world; but I left word before I left England a fortnight ago to have one of these meters finished and sent to Sir William Thomson, who was kind enough to say that he would test the instrument. The sister meter to this one was sent. This meter has an adjustment to it, which no manufactured article ought to have and no manufactured meter will have; but this being in the experimental stage, it had an adjustment which regulates the depth at which the first wheel works into the principal pinion. Now it appears that that screw had become loosened in traveling, before Sir William received it, and this fact was not ascertained until after the experiments were made; but the operation is now being repeated. I have received the tests made under these adverse conditions, which I will write down on the board. He applied different currents and he obtained the ratio of current to speed at these different velocities. He did not try to ascertain the feeblest current with which it would start. He began with one ampere and the ratio of current to speed was 66.5; with 1.5 amperes the ratio was 45.5. I will leave out the decimal place if you please. He then used two amperes and the ratio of current to speed was 44. He then used three amperes and the ratio was 39. He then used five amperes and the ratio was 39. He then used ten amperes and the ratio was 42. He then used 15 amperes and the ratio was 42. Although I look at that as very bad indeed, I think you will agree with me that it gives you a very good result. I would have been very well satisfied with that six or eight months ago; but I confess I am not satisfied with it at present, when we are able to get it so much more accurately. However, I thought it would be worth while to give you these results as obtained by Sir William Thomson. After the instrument had been running (it was one of these instruments with a large mass of metal in it) Sir William Thomson found what I have spoken to you about, that putting through the lower currents after it had

been running for a long time with 15 amperes; it was too favorable to the supplier of electricity and not sufficiently favorable to the consumer. After that he put it on to 10 amperes and he got only 37 as the ratio of speed to current, instead of 42, as it was originally.

I think that is all I have to say about the meter. I have no doubt that in the progress of discussion there may be points of interest raised, which may give me an opportunity to say something more about it, but I think I have really touched upon all the chief points of interest connected with it. (Applause.)

Prof. Forbes then passed a current through the instrument, and a recess of ten minutes was taken to enable those present to examine it. When the session was resumed, the President said:

As we have now had an opportunity to examine the instrument, the discussion will be in order, and the better ventilation the subject has, the more profitable will be the evening. We have with us this evening some few gentlemen who are not members of the Institute, and I beg to assure them that any questions they may desire to ask, and any information that they can afford us, will be heartily welcomed.

DR. VANDER WEYDE: Is it the intention to have that apparatus under the glass shade continuously while in operation?

PROF. FORBES: At the present moment I intend to use that glass shade, although I have no great preference for it. If any one has any objection to it, a metal cover is just as good, only one would need a glass window to read it by.

DR. VANDER WEYDE: The objection would be that when the apparatus is in operation a little while, the air will be heated and the ascending force would diminish and become very small, at least, when the air was considerably heated in the glass shade. To correct that, nothing would be easier than to make an opening in the top of the glass shade to let the hot air out and keep the surrounding atmosphere more uniform. The apparatus would give an indication of less velocity if the air is made warm, than if it is cooler. When the apparatus has been in operation for some time, so that the air which surrounds it becomes heated, the indications would become a great deal less. I wish to do justice to Professor Forbes for the ingenuity of the apparatus, and the remark I made is only for the purpose of suggesting something which may make the apparatus more perfect.

PROF. FORBES: These are very suggestive remarks that come from Dr. Vander Weyde, and of course they occurred to me during the process of the experiments. There is one lesson which I have

learned from this instrument, which is that you cannot calculate on what is going to happen with a convection current. I have been as sure as possible, over and over again, when I was making a minute change in the instrument, that I knew as well as possible what it was going to do—and it never did it. Dr. Vander Weyde suggests that when the air at the top of the shade gets heated, the instrument will move slower. The only answer I can give to it is that it does not. It goes perfectly, continuously, however many hours I have it at work. He suggests that a hole should be made at the top to keep it cool. Nothing in the world will ever induce me to let a hole get into that mechanism, any more than I would let the works of my watch be exposed to the open air to permit dust to get into them. But it is quite unnecessary, for it goes any number of hours at a perfectly constant rate, even when the top gets hot. It is remarkable that the first person who has spoken at this discussion has raised the first objection which every person who has ever spoken to me about it has raised, and that is, that its indications will depend upon the external temperature; that when the external temperature is high it will go quicker. This is the only one thing, however, in which practice does agree with theory in regard to convection currents. The theory of the thing is perfectly simple. The moving force is the difference of temperature between the conductor and the external air. It does not depend in the least on the absolute temperature of the place; it is only on the heat that is being generated in the conductor, and it is the difference between the temperature created there and the external air which gives you the moving force. It is a matter of indifference whether you start with a temperature of 30 or 100. As a matter of fact the indications in a warm atmosphere and a cold atmosphere are identical. I may forestall the question that is going to be asked immediately—whether the barometer has any effect. I may say that I have not had the instrument under an air-pump to test it, but I have tested it day after day and I have seen no perceptible variations.

DR. VANDER WEYDE: No person of any information would talk about a barometer in connection with it; but what becomes of the heat which is continually generated?

PROF. FORMES: It is radiated from the surface of the glass. A great deal collects at the top. A fixed temperature is arrived at, of course, and the heat which is being created is all radiated from the surface of the glass.

DR. VANDER WEYDE: It must be confessed, however, that heat is generated continually. Now, if experience proves that that does

not affect the instrument, that is all right; that settles the matter; only the suggestion is very natural.

PROF. FORBES: Oh, certainly, most natural.

DR. VANDER WEYDE: Because we all know that all the ascending force of air depends upon the difference of temperature between the air which is passing under the vanes and the air at the sides; and that the heat accumulating in the top must gradually descend and cause an influence, I cannot help believing.

PROF. FORBES: You are able to test it when you are passing a high current through it; you are able to test it by holding the hand on it.

DR. VANDER WEYDE: Putting the hand on it is not a good test. We have thermometers which can determine it correctly, and I cannot see why a small hole in the top would do harm. The gentleman said he would as soon expose the works of his watch to the air as to have a hole in the top of that shade. But I would like to be informed what injury a small opening in the top would be, to let out the hot air which must come up from below.

MR. FRANK L. POPE: The amount of heat which is generated by the action of the current is not very great at the most, and as it gradually collects in the top of the glass, it is probable that before the hot air extends down far enough to affect the operation of the instrument, that the radiation has become great enough to carry away an equal amount of heat with that which is being generated, and the temperature from that on is stationary, and that the balance is brought about before the currents of air are affected sufficiently to interfere with the operation of the instrument.

DR. VANDER WEYDE: But I suggested that the radiation through glass to the exterior air is very small.

MR. C. O. MAILLOUX: If it is asked what the objection would be to a small hole there, it seems to me that it is proper to ask what good it would be. When Professor Forbes has told us that the instrument works absolutely perfect without the hole, I do not think it is necessary to consider the absence of a hole there.

There are some other points which I would like to bring forward, and which, perhaps, may be of greater importance than a small hole in the top of that glass shade. One is the friction of the apparatus. I would like to know whether the calibration of the apparatus does not depend for its constancy on the friction of the wheel-work remaining constant as nearly as possible, and it also occurs to me to suggest that the friction of the apparatus may have a great deal to do with its sensitiveness as an integra-

ting instrument. In other words, if there is very little friction, it is quite likely that the inertia would be greater, and its momentum once obtained would tend to carry it over, so that if there were any abrupt changes in the current it would not so readily respond to them. If, then, we were to draw a curve of variations of current, it would not in reality follow that curve closely, but only partially, introducing a quantity in the result which would be the analogue of self-induction with an ordinary apparatus. Now there is another point. We all know that in order to obtain heat from a conductor we must have a certain amount of resistance, and I would like to know whether the amount of resistance that has to be introduced in the wire (which I presume is iron from its appearance) is of material consequence—whether it would figure at all as an element of resistance of the whole circuit, and also whether, by virtue of its resistance or of its position, it would introduce any variation, which has been called “impedence,” in the circuit in consequence of self-induction or anything like that.

PROF. FORBES: The first question is, as to the effect of the constancy of the friction—whether the speed of the instrument depends upon the constancy of the friction of the solid parts; and the second question I had better take in connection with it, which is whether that friction of the solid moving parts renders it less sensitive. First, we have to consider whether after the lapse of months or years,—when the mechanical parts of the instrument are not in such good order as when it was started,—its readings will be as accurate as they were at the beginning, and in the second place will it be so ready to start with a feeble current with the friction of the wheel-work as without it. I have already said that the most important part of the friction in that apparatus is the air friction; but that is especially so in the case of a larger current. When the currents come to the average size or greater, the air friction is, out of all comparison, the greatest friction that we have in the instrument. At any time the friction of the wheel-work, incredible as it seems to most people at first sight, who are not accustomed to thinking much of the subject—the friction of the wheel-work is but a fraction of the friction of the jeweled cup upon the pivot. You must remember that the first wheel of the wheel-work is rotated at only one-tenth of the speed of the mill-wheel, and its weight is extremely light compared with the weight of the mill-wheel, and the friction of the first wheel is therefore very slight indeed, and when you come to the second wheel, its speed of rotation is diminished tenfold and its lightness is small compared with the mill-wheel. I was told to expect this by a

great friend of mine whose name I ought always to mention in connection with this instrument—Mr. Stroh, who assisted Sir Charles Wheatstone in all of his celebrated inventions, and who has assisted me in all this work, and there is no man capable of giving such assistance in the mechanical details of an instrument like this. I had intended to make a German pun—a thing I never did in my life before—and should have done so if it were not for the earnest objections of Mr. Stroh, and call this a *Stroh-meter*. (Applause.) That is the way that the friction of the apparatus comes in. It is very slight indeed compared with the air friction, except with very feeble currents indeed. The result, then, has always been that what I call the ratio of speed to current was constant, whether the friction was a little more or a little less, but that it did not start quite so easily with extra friction; and I have no doubt that if the wheel-work were to get seriously out of order and we were to have very much more friction, it would not be so sensitive to a single lamp, but at higher speeds there would not be the slightest difference in the indications. For example, to show you how that works, I passed around two diagrams, I think, giving you the ratio of current to speed when different currents were passing through, with the clock-work attached, and without the clock-work attached. You will notice in those that, with the clock-work attached, it did not start with so small a current, but when the instrument was once in working order from one and a half amperes or so, or upwards, the two lines of ratio of speed to current were identical. As to the resistance of the instrument, I am asked whether it would offer a sensible amount compared with the resistance of the lamps. As soon as it does that, we must of course stop using it. I have never been able to raise this wire to such a temperature that it did not indicate proportionately. I have had my wires red hot, and up to that point it was still indicating proportionately. You must simply stop using it when the resistance here compared with the lamps is something you do not like. There is another question asked which raises a subject that I ought to have spoken more fully of in my opening remarks. Mr. Mailloux has spoken about Mr. Oliver Heaviside's impedance. [Mr. Mailloux had pronounced the word *impedence*.] I beg Mr. Oliver Heaviside's pardon. He has distinctly asked us not to speak of it in that way, but to call it *impedence*. He asks me whether the effects of self-induction are perceptible in the instrument. Now there are two effects of self-induction in a coil like that; one is the effect of induction of one turn of a spiral upon another, and the other is concentration of the current in all parts

of the wire, not allowing all the current to pass through the whole section of the wire equally. These are the results of using an interrupted current or any variable current. Both these effects come into play. In this case I have only four turns of the spiral, and their mutual influence is of course quite insensible. Many of you I presume read, with interest equal to my own, the delightful researches of Professor Hughes on self-induction, a year and a half ago, which he gave to the Society of Telegraph Engineers and Electricians in London. You will find recognized there the fact that a variable current passing through a conductor does not pass equally through the different sections, and it has been more generally recognized since that time than previously, that there was a greater resistance in a given wire to a variable current than to a steady current. Professor Hughes undoubtedly showed that this change of resistance is far greater in iron than in other metals. The metal I use for my conductor is iron. I admit that, having these experiments in mind and having followed out Professor Hughes's experiment myself in the laboratory at a considerable distance, I did expect to find a change in the resistance when I used an alternating current; and I expected that to use the same instrument for continuous and alternating currents would necessitate the employment of a constant by which the readings should be multiplied in order to suit it for the alternating current. I confess with the utmost gratification that on testing it by means of the Siemens dynamometer on the alternating current, and using the same Siemens dynamometer in testing it for the continuous current, there was absolutely no difference in the readings of the instrument so long as the current remained constant, whether the currents were alternating or continuous. Then I remembered that all Professor Hughes's experiments, and all those I had made in continuation of them, were with a made and broken current, in which the rapidity of change from no current to the maximum of current is terrific, and that very rapidity of change is what produces the spurious resistance—that apparent greater resistance when the broken current is being used; and that when we are using an alternating current which alternates very rapidly, it is true, but in a variable manner, in a manner whose variations are not commensurate with the rapidity of the variations in the interrupted or broken current, then those effects which were so startling in Professor Hughes's experiments do not appear sensibly. At any rate, as a matter of fact, with alternating currents and continuous currents the instrument reads identically. No constant requires to be introduced.

MR. S. S. WHEELER: I would like to ask what the resistance of the coil used is.

PROF. FORBES: Nearly a tenth of an ohm.

MR. WHEELER: And I would also like to ask if you cannot overcome the effect of induction by making one coil reverse upon another.

PROF. FORBES: Perfectly; but there is no effect of induction.

MR. GEO. D'INFREVILLE: The instrument being so sensitive, is it not desirable to take some special precaution to prevent it from registering when no current passes through it; for instance, put it in some place where it will not be subjected to a current of air, or the radiation from the sun, or the effect of hot air?

PROF. FORBES: No, I cannot say that it is. When it is under a shade, as far as I remember, I have no recollection of ever seeing the wheel moving when there was no electric current passing through it. I am pretty sure it never does move; but you must remember it requires to go round a thousand times before it registers a sixpence worth; so even if there were a few turns it would not matter a bit.

MR. HENRY GREER: I would ask if this has ever been introduced into an Edison current?

PROF. FORBES: What do you call an Edison current? (Laughter and applause.)

MR. GREER: I mean the current that is working in that system bounded by Wall street, Nassau street, Beekman street and— (Laughter.)

PROF. FORBES: This is the first time the instrument has ever been at work in America.

MR. MAILLOUX: I want to say that there is one point in the questions which I asked, that Professor Forbes did not fully answer, though he has since nearly answered it in reply to my friend Mr. D'Infreville. It is in reference to the sensitiveness of the machine as an integrating instrument. In other words, if you take the rates of variation of a current that is not varying at too rapid a rate, and then a current which we will say is comparatively steady, but which is suffering some periodical variations, somewhat like this—(illustrating on the blackboard)—if we draw another curve in which the rate of speed of the mill-wheel is used to represent corresponding rates of current, then evidently when we come here (indicating)—if that has more or less inertia, then we will find that the increase of speed of the mill-wheel will be a little more, and then we will find that it will dally a little behind, and that here also (indicating) it will be a little slower. In other

words, we would find that perhaps these waves would not be followed so closely. That is what I meant by impedence and the factor which would be the analogue, though not the exact equivalent, of self-induction.

PROF. FORBES: The momentum of the apparatus has no effect. The momentum is extremely trifling. As soon as the momentum tries to carry it around at a different speed from that which is determined by the velocity of convection currents, these vanes are pulling every way through the air, and act as a drag on it, and it immediately suits itself to the velocity of the air currents. Since that question has been raised I would like to say this, that you must not expect the instrument to go jumping about at a very rapid rate with every change of lamps. With a small current it takes about five minutes to settle down to its readings. With the large currents it takes about a minute. So that really, if these were the variations in the current (illustrating), in all probability the reading you would get would perhaps not reach its maximum quite so soon, and here again it would have a curve whose variations would not be quite so great.

MR. MAILLOUX: That is exactly what I mean.

PROF. FORBES: That is not due in the least to any momentum in the instrument. That is due to its taking a sensible time to accumulate heat in the wire and a sensible time to get rid of the heat accumulated in the wire.

MR. SIDNEY F. SHELBOURNE: The Professor said it took about five minutes to get its rate of motion in the mill-wheel with a feeble current. Now, in regard to the first question that was asked by Professor Vander Weyde in respect to making a hole in the top, does it not appear that the apparent inaccuracy of the instrument in registering light currents, and in getting its rate, may be due to the generation of heat at the commencement, and that heat accumulating in the top of the instrument and heating the glass, so as to arrive at a point where there is an equation between the generation of heat by the wire and the radiation from the outside of the glass? I simply suggest this, and I ask the Professor whether he ever has tried in making instruments, instead of making the whole of the cover of glass, to put a dome of copper or some other conducting metal upon it, so that the air which rises and accumulates at the top may sooner reach that equation of radiation from the outside of the instrument with the generation of the heat which operates the mill-wheel?

PROF. FORBES: I am asked as to the inaccuracies of the instrument with a feeble current, and the cause of it. I confess that, as

a user of the meter, I have not the slightest objection to its not indicating at its full speed for the first five minutes, when you are using very feeble currents. The monetary value of such a thing is imperceptible. If we were using the instrument to determine the true value of the ohm or any of those profound researches, it would be right to go to very great accuracy. But I do not think it is necessary to go to any greater accuracy than we have at present on that point, and to make the feeble currents begin their accurate reading a little sooner. I really do not think that practically it is very important. Otherwise, I think the suggestion that was made might very likely be good—using a metal shade. Of course I have had numerous changes in shades. I have tried a shade which was nearly all metal. But I found this shade before you the best one I have used up to the present time. However, I think it is quite likely that the suggestion just made might be a good one—that a metal shade would be fully as good as the other—might be better—might make it a little more sensitive, perhaps. I think there may be a good deal in what you say.

MR. JOSEPH WETZLER: I have no doubt that the instrument in its present shape has gone through sufficient experimentation, as to make any suggestion regarding its history a little out of place. But I am a little interested in knowing whether any experiments have been made to determine the best angle of those vanes, and the best shape of them, and if the shape determined on there has been found to be the best.

PROF. FORBES: As to the angle, I have committed the grossest folly that a man with the experience I have had with this meter could commit—I have trusted theory. I have been positively certain that an angle of 45 degrees must be the best. I am sorry Mr. Wetzler has drawn this confession from me, because it is really the only respect in which I have trusted to theory. As to the other points, the shape of the vanes has been the source of endless experiments. I made an enormous number of changes before I arrived at this shape, and even little details in the shape have come from experiment. I made hundreds of vanes in the experimental stage, and I am pretty sure that if that type of mill-wheel is to be used the vanes are the right shape.

MR. ELIAS E. RIES: Professor Forbes mentioned at the outset that his attention was drawn to the devising of this meter by the fact that no meter existed for measuring alternating currents. Now, during the elaborate discussions we have listened to this evening, nothing was said about its application to alternating currents. Many would like to hear from Professor Forbes as to its

use in that connection, and if the meter is specially adapted to alternating currents where other meters heretofore devised are not applicable to that purpose.

PROF. FORBES: I should be very pleased to answer any such inquiries where there are points which I have not touched upon sufficiently. Meters in the past have been constructed by the use of, I think, only two principles of electrical action; the one being electro-chemical action and the other electro-magnetic action. Several instruments have been devised with electro-magnetic action as their basis; but those which were sensitive enough to be practical necessarily contained iron in their construction. I do not know of any meter which has been brought into practical form which does not depend on one or the other of these two principles—the electro-chemical or the electro-magnetic principle. With an alternating current, the current is, of course, alternately going in opposite directions, and in any electro-chemical arrangement which may be used, whatever is done by the current when it is going in one direction is undone by the next alternation of the current. So that it seems to me hopeless to think of designing a meter based upon electro-chemical action. The designing of a meter based on electro-magnetic action is far more hopeful; though even in continuous current meters I believe the difficulties have not been altogether gotten over; but in using alternating currents the phenomena of self-induction come in, and not only that, but also the fact of your having iron. The intensity of the magnetization of the iron depends upon the speed of the alternations; so that, as a matter of fact, it has been found in every meter which has iron in its construction, that the indications of the meter, even if you keep the current perfectly constant, are totally different when the speed of the engine varies; that is to say, when the number of alternations per second is changed. So that if you were testing such a meter, you would test it at one speed and you would get at its constant—if it had a constant—and then you would give it to your consumers, and of course you would not run your engines at that speed; you would run at a totally different speed, which would favor you as suppliers. I think it is immoral to suggest such a thing as that; it lends itself to fraud. That is the objection to the electro-magnetic meters, that they give totally different indications even with the same current, when the speed of alternations is changed.

MR. F. L. POPE: Like a gas-meter when you change the pressure.

PROF. FORBES: Yes; like a gas-meter when you change the pressure. It was thinking of these things, and thinking also of

other effects of the electrical current, that made me conclude that the only thing which would do was heat. I have often thought of the question of meters before, and although your president has been kind enough to say that I have invented one or two things, I always try not to invent, if possible, because that is not my business. My profession is consulting work. But sometimes it is irresistible, and when a man defies you to do a thing which you know you can do, it is totally irresistible. I had been thinking over the matter a good deal before, but I never had made up my mind how I was going to do it; but so soon as this defiance was put before me I immediately hit upon this method. The reason, then, why this is suitable to alternating currents is that it measures the heat generated; it measures the energy which is used up; it measures a certain fraction of the energy which is being used up in the lamps, and that is what we want to measure, and it will do so whether we are using alternating currents or continuous currents.

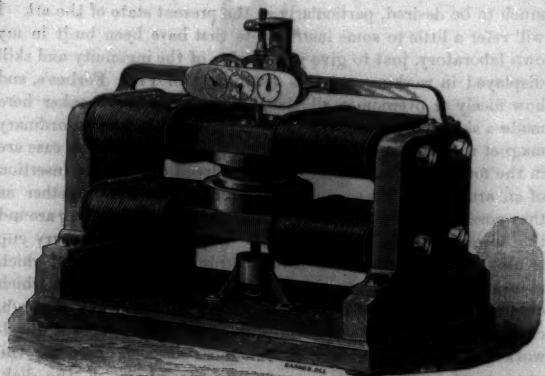
MR. RIES: The speed of the alternations per second has no effect?

PROF. FORBES: Has no effect upon this. This simply measures the energy—a certain fraction of the energy of the lamp.

THE PRESIDENT: We have with us this evening a gentleman who has done more than anyone else to elevate the standard of electrical instruments of precision in this country. I allude to Mr. Edward Weston. The subject before us has already developed many points of great interest, and I have not the least doubt that Mr. Weston can contribute very largely to our information on these points and also touch upon others that have not yet been brought forward. Mr. Weston, we should be very happy indeed to hear from you.

MR. EDWARD WESTON: I do not know that I can say very much more than has already been said in regard to the instrument by Professor Forbes. I think it is an extremely ingenious and beautiful instrument. There has been already a great deal of time and a large amount of money spent on electric meters for direct currents, and I can say with Professor Forbes that I do not think we have had any current meter of any value whatever. The electrochemical system in use by a very prominent company here was put out with a great deal of confidence in the results to be obtained from it. I think anyone looking at the principles involved would condemn it from the start. You are to take a small fraction of the total current, and a very small fraction indeed, and pass it through an electrolytic cell, the diameter of which varies from

time to time, and the resistance of which is never fixed, and pass the bulk of the current around a resistance which is practically fixed; the change in the resistance of the metal would be but a small proportion of the change in the resistance of the electrolyte. Now, in such an instance as that, you are measuring a very small fraction of the current passing, and whatever error you have, you are multiplying that error by that fraction. If you are measuring 1-1000, you are multiplying your error by 1,000. Anyone who has had much to do with electro-metallurgy knows that the cells are constantly changing; that to keep the deposit regular and uniform, to keep the resistance of the solution uniform, they must be constantly attended to. There is not an electroplater in the coun-

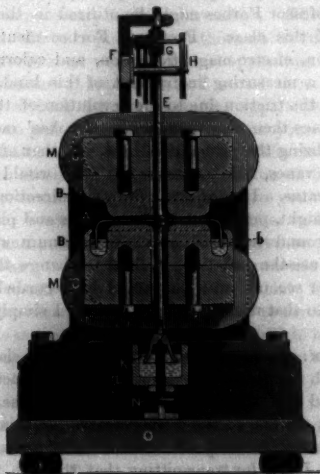


THE WESTON METER. (Fig. 1.)

try who does not once in a while go in and stir up his baths. Such an instrument is of no real value. We may, therefore, I think, dismiss those instruments entirely from consideration. A great deal of work has already been done on magnetic instruments; that is, instruments which depend upon the magnetic attraction of the current through a wire—or the current through a wire going around an iron coil. In those instruments we have all the defects which have been referred to as being possible to exist in this instrument here to-night, manifested a thousand times more, with numerous others that do not exist in this instrument

at all. I can speak feelingly on the matter because I have given a great deal of attention to it, and have thought once or twice I had something very good, and have been finally obliged to abandon it. If anyone will take the records of the Patent Office on electric meters, he will see there has been no small amount of time, and thought, and money, spent on that question. Practically, we are in the same position we were before. We had nothing except the electro-chemical meter, which, as I say, I consider entirely worthless. Instruments have been built which are approximations, but nothing in comparison with this, and if they were, they would not be of as universal application as this instrument is. This instrument is applicable to either alternating or direct currents with the same practical degree of accuracy—a thing very much to be desired, particularly in the present state of the art. I will refer a little to some instruments that have been built in my own laboratory, just to give you an idea of the ingenuity and skill displayed in working out this instrument of Prof. Forbes's, and how nicely it overcomes all the difficulties. [The speaker here made a sketch Weston meter, Fig. 2.] We have here an ordinary magnet with the so-called consequent poles, which in this case are in the form of disks, instead of being curved out for the insertion of an armature. These poles are brought as close together as they possibly can be. There is an annular chamber running around the lower pole, in which mercury is poured. A mercury cup is also placed on the base of the instrument, in the end of which is inserted a cork float to get as little weight on the pivot which enters the stem as possible. This stem passes up right through, connected with the disk at the center and then engages a wheel-work similar to that which Professor Forbes has here for the purpose of registering the number of revolutions of the disk. Now, if this magnet, which was wound with very fine wire, was placed and so proportioned as to be saturated with the maximum current that would be likely to flow with the difference of potential on the circuit, *i. e.*, if the core was kept constantly saturated, then a current passing in would go through the disk readily, down through the cup and out through the frame of the magnet, the magnets being placed in derived circuits so as to keep them constantly in the point of saturation. Under those circumstances, of course, we should have a series of radial currents flowing out from the center to the periphery, or vice versa. Such being the case, the disk would commence to rotate and record the number of revolutions on the little wheel-work or registering apparatus employed with it. This instrument was the result of a great deal of hard

work. It has been absolutely abandoned. I could not find anything that was really satisfactory, and I came to this last of all. It stands on my laboratory table now. It has not been touched for two or three years. This instrument, however, within certain limits, was really admirable, but those limits were so narrow as to entirely prohibit its use for practical work. When the speed varied, the element of friction in the mercury came into play here, and very seriously changed the rate of revolution of the instrument. I do not know whether Professor Forbes has done any work involving the friction between a mass of metal revolving in mercury or not.



THE WESTON METER. (Fig. 2.)

PROF. FORBES: I did a good deal of that at one time.

MR. WESTON: If you did, you will understand the difficulty. Nothing was accomplished. Now when I come to see an instrument of this kind, after the amount of work that has been put on mine, it makes me ashamed to think that I spent so much time on a thing of that kind. I may say that it has already given rise to a very neat form of current instrument in which, instead of having this disk rotate, it was fixed, and a large index brought out

from the disk, in which case it was absolutely proportional to the number of lamps in circuit. For a station ammeter I do not know of anything that equals it to-day. It shows one lamp when there are 300, or 400, or 500 on, just as it shows one lamp by itself. Professor Forbes has got here the simplest instrument that I think could be built. I do not see how you could improve it very much except in detail. The principles are correct, and I must congratulate him very highly indeed upon the success of the instrument. (Applause.)

MR. REIS: After Doctor Vander Weyde's suggestion, which was the first one made this evening, about that hole in the roof, it occurred to me that perhaps another principle besides those mentioned by Professor Forbes might be utilized in the construction of a meter of this class. Professor Forbes mentioned electro-chemical action, electro-magnetic action, and caloric, or heat, as the basis for a measuring instrument of this kind. Now if we could obviate the friction due to the revolution of these vanes in the air and place them in a vacuum like Crookes' radiometer, and instead of utilizing the principle of heat, use that of light to give motion to the vanes, I do not know but that would make a very delicate apparatus. It suggests a different direction in which to work. You might, perhaps, take these vanes and place them in a dark cell, surround them with a strip of platinum wire or carbon filament, and use the variations in light to change the number of revolutions per second, and that could be recorded by an apparatus similar to that used by Prof. Forbes. I simply offer that as a suggestion.

MR. WESTON: I did not speak of the general class of meters. There are such meters as are dependent on the action of clock-work. A good deal of work has been done on those. I think Mr. Gardner took out one of the first patents. In this case I think the clock-work was to keep the gear-wheels moving at uniform rates, and the device for transmitting that through the recording apparatus was to enable a change to be made in the rate of recording. Of course, there has been a great variety of instruments of that class designed. Then there has been a class designed which has contemplated the quantity of gas generated by electrolysis. That can be used: but the same objection holds in that that holds in the other one, that you cannot put that instrument in the direct circuit. It must be in the derived circuit, and you must then measure the gas, which induces a great many errors which have to be compensated for. Then there have been other instruments in which it has been proposed to use strips of paper

running under a stylus which could be moved more or less by a magnetic current. But you can see, when you sift them all down, that there is not one which is applicable to any system of work that is now in use. This instrument appears to me to be so applicable. Some people may say that is a very delicate instrument. Well, there is a great deal of very delicate work about electric lighting, and the sooner we get men who are delicate enough to handle it, the better we shall be off. You cannot wait for men to be educated or depend upon men entirely. You have got to run on with the art and the men must grow up with it. A few years ago there were no ammeters. Volt-meters used to be ridiculed. Everything was rule of thumb. People are beginning gradually to see that it is necessary to use instruments. They find it on the balance-sheet. I would just as soon think about running a manufacturing establishment where electrical apparatus is made, without delicate measuring apparatus, as I would think of running a machine shop without delicate measuring apparatus. It is just as foolish to try to get along with a foot-rule as your standard in a shop, and without using standard gauges. The man who works to 1-4000", which should be the absolute limit on all first-class work, is the man you want to employ. He will save you money, because he will do your work the same way every time, and there will be no tinkering and pottering to get things together. (Applause.)

THE PRESIDENT: It is not at all our practice to call upon gentlemen to speak; but I must say that I think I do but utter a general desire when I request Mr. Howell to favor us with a few remarks. He is working in a department which has been more than once alluded to this evening, and with which he is very well conversant.

MR. J. W. HOWELL: Mr. President, I was very much surprised to hear what Mr. Weston said. I am surprised that he does not know that there is a temperature correction in the Edison meter. The Edison meter has been in use industrially for five years, and I say without hesitation—and I am in a position to know the opinion of the practical men who are using the Edison meter—that they think a great deal more of it to-day than they did before, and the more they use it and the more they know of it, the better they like it.

There is one thing about that meter of Professor Forbes's which recommends it to me as the result of experience with instruments sent out for general use. There is one peculiarity about every American electrician—he has got to know what things are made out of, and if you do not show him he will find out for

himself. Now that instrument shows itself. You can look at that and see what every part is, and you do not need to take it apart. I have sent out a great many instruments. The first thing a man does with them is to pull them apart and see what they are made of. There is no necessity for pulling that instrument apart—you can see all about it.

There is one thing I would like to ask Professor Forbes, and that is the effect of lowering the shade. I see he has a shade there very much higher than the instrument apparently needs. I would like to know if that is necessary to allow for air circulation.

PROF. FORBES: Yes; it is more sensitive with a higher shade.

MR. HOWELL: You find that that shade is about the right height?

PROF. FORBES: That is about the limit. I have gone through dozens of shades to see what is the right size and shape, and that is the one I fixed upon.

MR. HOWELL: The impression that instrument makes upon me, and upon a good many I should think, is that there would be a difficulty in shipping it and getting the men who have to put it up and use it, to do so properly. I know that the men who will use those meters—the men who do our meter work in electric lighting—are not men in whose hands I would like to put an instrument of that sort. How is that shipped may I ask, Professor Forbes?

PROF. FORBES: I packed the glass case separately when it came over. On this occasion I took off the vanes. I had three parcels for that instrument—one containing the clock-work and all the brass-work you see there; another containing the mill-wheel, and another containing the glass shade. They have traveled in England very large distances with that arrangement in action, which I pointed out before, by means of which the vanes are pushed up against a stop. The person who was going to use it had nothing to do except tightening up a screw as hard as it could go, and then it was free to work. There was absolutely nothing to be done except that, and I think I should do that in future if I replaced that glass shade with a metal cover. I have not replaced it with a metal cover, because you can see it work. I like the glass shade, but I dare say a metal shade with a window will, in practice, be the thing that will be adopted, in which case the whole thing will be perfectly safe to pack up in one piece. I shall certainly not allow the workman who uses it to touch the mill wheel. I shall have that arrangement which is on it now for lowering the vanes on to a point.

MR. WESTON: I take it, gentlemen, that the instrument exhibited here is not intended to be the instrument in use. We know that people do a good deal to get around meters. Gas companies will tell you that people will punch a hole through. But of course it is practicable to protect that as well as we can protect anything.

In regard to the electrolytic method by the direct current, I think I am aware, and I have been aware for a long time, that Mr. Edison has had a temperature correcting device in circuit that will work within certain limits. I still say, and I say it without any fear of being proved incorrect, that such a meter cannot work successfully and practically within the limits that it ought to work, and I think that is the universal testimony of consumers at any rate. I suppose there will always be more or less complaint by consumers about all kinds of meters, but I think they have been pretty well founded in that case, because I think the meter is incapable of doing as good work as we want a meter to do. I do not mean to say you cannot make a meter that will work.

MR. CROCKER: I ran roughly over the amount of heat that would be generated in Prof. Forbes's instrument, and it came so high that I should like to run over the figures now and see if they are right. He says the resistance is a tenth of an ohm.

PROF. FORBES: Under a tenth of an ohm I said.

MR. CROCKER: Assuming it to be a tenth of an ohm, 20 amperes would give there two volts drop, and two volts and 20 amperes give us 40 watts, and 40 watts multiplied by $44\frac{3}{4}$, which is the only equivalent which I happen to remember, is about 1,760 foot pounds, and 1,760 foot pounds per heat unit, Fahrenheit, gives us two heat units roughly. That is, two pounds of water will be heated one degree Fahrenheit in one minute by Professor Forbes's resistance when it is working up to its full capacity of 20 amperes. Two pounds of water heated one degree is equivalent to eight pounds of air heated one degree. I believe the specific heat of air is a quarter that of water. That is my recollection; it may be wrong. There are eight pounds of air heated one degree Fahrenheit. Now the contents of that shade there is approximately a quarter of a cubic foot. I should think it is a foot high and quarter of a square foot in cross section. Now there is one quarter of a cubic foot of air. One cubic foot of air weighs $\frac{1}{16}$ of a pound. A quarter of a cubic foot weighs $\frac{1}{64}$ of a pound—a fiftieth of a pound—the contents of that shade. Therefore the heat produced in that shade would be fifty times as much as it would be in one pound of air. Eight times 50 is 400. Therefore we have 400 degrees per minute. It is

a very high figure; I cannot believe that it is right. But we know that the capacity of the amount of air in that shade for heat is very small, and we also know that the amount produced in a tenth of an ohm by twenty amperes of current is quite large. Therefore perhaps that figure is approximately right.

PROF. FORBES: I think perhaps it would be better if I might be allowed to answer the question after it has been written out by the shorthand writer. It is rather difficult to follow the point where we went from water to air and the larger quantities, and to see that the figures were right. But I do not see that the quantity of air that is heated or the quantity of water that is heated has anything to do with the question. I would suggest that the speaker, instead of taking that line, would simply multiply it and find how much energy is used in the meter when twenty lamps are burning, and find out how much energy is used in the lamp when twenty lamps are burning, and divide one by the other and get the percentage. Settle upon the percentage more than which you are not going to use, and you have it. I should think very much over two per cent. or so would be very undesirable to use. In some cases it might go up to three or four, possibly, but I should think as a general rule you would find two per cent. as much as one would care to use. The question of the number of lamps you use simply depends on what amount of energy you care to use.

MR. CROCKER: I did not mean the efficiency of the apparatus. I did not mean that Professor Forbes's meter used up any considerable fraction of the current. I merely meant to bring forward as an interesting point how much heat is produced in the meter in a given time, and to calculate it as well as I was able to with the figures I happened to have in my mind.

PROF. FORBES: Then I would point out that theoretically you have to take in another point, and that is that there is continual radiation going on, and that during constant running there is no addition to the heat contained in the instrument. It arrives at a constant condition when the heat generated in the wire is equivalent to the heat radiated from the surface of the glass. From numerous experiments both in Sir William Thomson's laboratory and also by Professor Tait we have got at the constant of emissivity, the rate of radiation, and it is very easy to go on with the calculation a step further than done just now and calculate what is the temperature which would be reached when the emissivity or the radiation from the surface is equal to the heat generated from the wire. The problem, looked at simply as a theoretical

problem of interest, can actually be worked out by going a step further than the speaker has just stated.

MR. WHEELER: What is the approximate temperature of that globe after running a few minutes?

PROF. FORBES: Running at its full, it is 120 perhaps.

MR. WHEELER: Even at that low temperature it must get enough higher than the surrounding air to radiate $2\frac{1}{2}$ heat units.

THE PRESIDENT: While these calculations were being made as to the condition of the air in that globe, I was very forcibly reminded of the condition of the air in this room, and it made me wish for the time, not so far distant, I hope and believe, when both the civil engineers and ourselves will have a meeting hall worthy of the two societies.

The discussion being at an end, though by no means exhausted, I think nothing remains but to ask Prof. Forbes to close it with such remarks as occur to him as proper, and that being done we will adjourn to the next room, where a collation awaits us.

PROF. FORBES: Mr. President, I really have very few remarks to make except to say that I feel that this discussion has been of great value to all of us. Very interesting points have been raised, and I have heard suggestions which have given me great interest indeed. Of course, in working at this instrument one started from theoretical notions, but one gave them up very soon and one had all sorts of resources at one's hand to vary the forms of these curves, some of which I have already shown you. I should like to show you the curves which I got when I first made this instrument. The curve ought to be simply a horizontal line. One of the curves which I first got was almost a U, and yet I had the greatest hope even when I got that. By two changes I was able to reduce the line down to a horizontal, and there were numerous other changes. In the course of working that instrument out, I learned more about convection currents than I knew there was to learn, and really one has power now to handle them as one pleases.

I have no more remarks to make, except that I thank every one here for the pleasant discussion we have had this evening. I think it has been very instructive, and I have felt throughout, the same kind, cordial sympathy that was shown in England. I did

not expect to find everyone ready to look upon it in the way in which Sir William Thomson did, without any sign of regret that it happened to be done by another. I thank every one here for the cordial way in which they have received this stranger. (Applause.)

On motion of Mr. Pendleton, a vote of thanks was tendered to Professor Forbes, and the meeting adjourned.



